without any intellectual impairment whatever. There appear to be no valid physiological or psychological reasons why this state of affairs may not exist due to disease of the brain. Still it is affirmed that these cases are not what they appear to be, that every case is either one of responsible moral depravity or that, whether detected or not, there exists some intellectual trouble of such a kind as to place it in the catagory of intellectual insanity. But until this is proven, and on those who make this wholesale affirmation that there is no such thing as moral insanity rests the burden of proof, I cannot positively refuse to admit that this form of mental disease may and does exist.

ART. II.—CONTRIBUTIONS TO ENCEPHALIC ANATOMY.

BY EDWARD C. SPITZKA, M. D.

I.

The Methods employed in, and Deductions admissible from, Cerebral Anatomy.

AS Henle has well said, the tracing of each nerve root offers sufficient occupation for any single observer, and the suggestion, thus thrown out, of a division of labor in the intricate and difficult field of cerebral anatomy, is certainly a good one. But as long as the various investigators in this field have not come to a mutual agreement, according to each monographist his separate and appropriate field, they will, as heretofore, be forced to content themselves with furnishing scattered and fragmentary contributions to the existing stock of knowledge. May this serve as an apology for the disconnected character of the present communication.

Neuro-anatomy boasts of more numerous, as well as more elaborate methods, and enjoys a far wider scope, than any other branch of morphology. In its methods it exhibits an intimate blending of the macroscopic and the microscopic; in its scope it includes not only the systemic relations of nerve-strands and topography of the gray masses, but also a comparative study of the brains of lower animals, the results of pathologically or experimentally induced secondary atrophies, as well as the embryonic development and teratological aberrations of the nervous system.

After hardening a hemisphere of the cerebrum in absolute alcohol, or in a combination of Mueller's fluid and methyl-alcohol, we obtain an insight into the coarser relations of the larger nerve strands. In studying the connections of the larger associating garlands (fasciculus longitudinalis sup. and inf. f. uncinatus), this is the *only* available method, and I must here insist, that in the hands of an expert dissector, who is able to supplement the defibrillation of these coarse fasciculi by a microscopic examination of their terminal portions, trustworthy and valuable results are obtainable, and consequently, take a most decided exception to Forel's objections to the method of defibrillation. It is certainly impossible to obtain the whole length of any bundle of fibres in the corona radiata within one microscopic section, for the simple reason, that not one of these bundles runs in an even plane. In the peduncular tracts, however, as well as in the basal ganglia, various fasciculi are so closely interwoven, and so frequently interrupted in their course by intercalar or foreign gray matter, that nothing less than an accurate microscopic study of successive sections will assist us in unraveling this labyrinth. It is not often our fortune to be able to trace any single nerve fibre through a large series of sections, but this is not as Forel seems to think, at all necessary. There is so much individual variation, that such painstaking investigation of individual axis cylinders might prove futile, in demonstration whereof I need but refer to the variations in the pyramidal decussation, in the development of the upper olive, as well as of the nuclei and fibrae arciformes, which latter are almost entirely absent in some subjects.

For the study of the anatomical relations of the peduncular tracts, I have for the last two years employed the following methods, which I can recommend.

The Island of Reil, basal ganglia, crus, pons, medulla, and cerebellum, are separated entirely from the cerebral hemispheres[1], and laid on cotton, in a flat-bottomed dish, filled with a solution of bichromate of potash. The object of this is to get the axis of the peduncular tracts into a straight line, in order that sections vertical to this axis may permit of an insight into the finer relations and connections of the nerve radicles. Sections made in any other direction fail to exhibit the latter in their whole length, and it is this fact which renders Gudden's sections of comparatively little value, for in them the nerve roots are not continuous, but interrupted. The preservative solution may be dense or dilute, according to the temperature in which the parts are to be kept. If the apartment is warm, rapid hardening is desirable, and the solution may be almost a saturated one; if it is cool, from one to two per cent. solutions can be employed, and a longer time is given to the hardening process. That the latter method yields by far the best results I need not add. If the investigator has the whole body at his disposal, he can ensure the best results, and not only accelerate, but also equalize the hardening process, by injecting the internal carotid and vertebral arteries, or either, with a two per cent. solution of the salt. This is continued under *exceedingly gentle pressure*, until the fluid running from the jugulars is no longer tinged with blood.

Where time or circumstances do not permit the employment of this circumstantial method, I usually do not preserve the peduncular tracts as a whole, but divide them into segments freshly. It is a matter of experience that these segments harden much more rapidly, and equally, than the whole ambitus, and a previously prepared series of sections, through an ambitus, hardened as a whole, will assist in localizing the altitude to which a section obtained from any part, separately hardened, belongs. For histological purposes, the sections obtained from parts hardened in bichromate of potash, to which a little chromic acid may be added, are preferable.

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[1] This procedure, first employed by Meynert, is now becoming a part of the autopsy routine, where accurate results are desired. I described it last year, in the introduction to a paper on the methods of making autopsies in insane subjects, read before the International Medical Congress, in Philadelphia.

Those hardened in alcohol stain too diffusely in carmine, and the absolute alcohol, which can alone harden brain tissue well, possesses additional disadvantages, which forbid its use in histological investigations of nerve tissue, namely, it extracts *leucin* and other chemical bodies from the nerve substance, and precipitates them diffusely throughout the basis substance in larger and smaller spheres, or even in the perivascular channels. Many descriptions of colloid and fatty granules in the brains of those dying insane, or even hydrophobic, have no other basis than these artefacts.

Having thoroughly explored the topography of the ganglia, nerve nuclei, strands, and roots, as these are seen in such a series of transverse sections, it becomes desirable to study the longitudinal fasciculi in profile, that their terminal points and origin may be determined. As a rule the purely sagittal section series will suffice for the determination of most longitudinal strands; for many, however, an oblique sagittal, oblique frontal (I always speak of the peduncular axis as if it were a continuation upwards of the spinal, which is far from being the case, but which renders our topographical designation far simpler than if we were to follow the inflections of the basi-cranial axis), or other direction is necessary. It would lead me too far in this place to describe the procedures necessary for each fascicular-tracing, but shall briefly refer to them when describing the fasciculi themselves.

Where a nerve bundle does not run in a straight line, or at least in a plane, I am sometimes able to represent its whole length in a single microtome section, by adopting the following procedure: the bundle is superficially exposed in its whole length by a cut with the section knife, made on the fresh brain, a wedge-shaped piece is cut out from the opposite side of the specimen, and then the cut surface, in which the desired bundle is, placed downwards, so as to become flattened on the floor of a flat dish filled with preservative fluid. The floor of the dish is covered with an even layer of cotton, or some other permeable material, as otherwise the preservative re-agent might not penetrate to the under (required) surface of the specimen. The object of cutting out a wedge-shaped piece is to remove the elastic tension on the intact surface, which would prevent our converting the natural curve of the surface required, into an artificial plane. After hardening the specimen, a microtome will furnish the desired sections containing the full length of the fasciculus.

To study the minute histological details of a given region it is desirable to examine fresh specimens, and small segments hardened in chromic acid or Mueller's fluid.

While by the above methods we obtain specimens, which stain sufficiently well, and exhibit the parts in a nearly natural condition of preservation, yet they do not possess that clear demarcation of fasciculi, and nerve nuclei, which is desirable in specimens intended for demonstration to students and beginners. Here histological niceties can be dispensed with, and the combined use of alcohol and bichromate of potash will yield the finest school specimens. The procedure which I have adopted in preparing objects for demonstration in my private courses, has been as follows: The ambitus is immersed in proof spirit for twenty-eight hours, from this it is transferred to a five grain solution of bichromate, on which solution it will float until it has become permeated to its centre by the latter. As aethyl alcohol and the chromic salts when combined give a green color, it is well to pour off the first fluid and replace it by a stronger solution every other day for a week. At the end of six weeks I wash out the specimen in water, until the latter does not take on a yellow tinge, and again submit it to the action of alcohol, the same routine is gone through again, only that now chromic acid in a one per cent. solution takes the place of the bichromate. The specimen is imbedded for cutting, after having been in alcohol for the third time. This procedure yields objects, which, seen with a *low* power, are far more instructive than those obtained from either alcohol or the chromic salts alone.

Whether we employ one or the other of the methods mentioned, a good microtome is essential to the obtaining of complete and even sections of the ambitus.

The best microtome, of which descriptions have been published, is unquestionably that of Gudden. Without any knowledge of his instrument, and in want of a better method than any with which I was at that time acquainted, I independently devised an apparatus which has worked very satisfactorily, and differs materially from the instrument of Gudden.

It consists primarily of a movable and an immovable portion, the former has the section-knife affixed to it, the latter holds the specimen to be cut. The description falls naturally under two heads:

A. The movable portion, consists of a large thick plate of glass 16″ square, two holes bored through it, receive and support, two brass pillars which, when the apparatus is in operation, stand on the lower surface of the glass plate. On its upper surface are two corresponding screws, by means of which the pillars may be tightened or loosened. Both brass pillars are beveled off on the free end at an angle of about 7.5°. It is essential that the angle should be as nearly the same on both pillars as the human hand can make it, for on this depends the equal inclination of the knife, which is fixed to these pillars.

The knife, which must be ground on a perfectly level stone, is about ten inches long, and fixed to the two pillars by screws. The degree of inclination which the knife assumes by virtue of the beveled off extremities of the pillars on which it rests, has been found by experience to be the best for a microtome knife.

B. The part that holds the specimen, consists of an oblong box with appliances for introducing or removing the water, under whose surface the sections are cut. At the floor of this box is a brass plate *whose surface must be perfectly parallel with the upper edges of the sides of the box.*[1] Here there are fixed appliances for attaching three brass cylinders, of a bore respectively of one, two and a half, and five inches. The plate is perforated in the centre for a large screw, with a regular spiral turn. To manipulate this screw whose *milled*

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[1] For the accomplishment of this, as well as many other difficult mechanical problems connected with the construction of this instrument, I am indebted to my father. The accuracy of the apparatus is such that when worked on a level table with the knife ground perfectly level, the deviation in a run of ten inches does not exceed one three thousandth of an inch.

head is worked *under* the box, the latter is raised on four legs, thus giving the requisite working room. A spring click under the box is placed, by the side of the milled head, and registers the movement upward of each one twelve hundreth of an inch with an audible click. This enables the operator to know the approximate thickness of his sections; thus two clicks would signify a thickness of one six hundreth; four, one of one three hundreth of an inch, etc. The imbedded specimen is pushed up by the microtome screws through the intervention of an accurately worked cylindrical piston.

The specimens are imbedded in a mold of paraffine and oil (9:1) cast in the respective cylinder. If care is taken to press the upper surface of the paraffine while it is becoming semi-solid, the very undesirable retraction of the cast from the walls of the cylinder will be avoided. Before I struck on this simple manipulation, I, like Forel, used wedge-shaped pieces of wood, or made a second cast to fix the loose paraffine cylinder. With Forel, I recognize and insist on the importance of not cutting any of the paraffine; there should be as little paraffine around the immediate part to be cut, as is consistent with its support during the cutting process.

The box is now filled with water to a height of at least one inch over the upper surface of the specimen. The glass plate is then placed on the upper border of the box with the attached knife downwards, whose edge is directed towards the specimen. It should be remarked here, that the knife does not touch the cylinder which holds the specimen at any point. That such a contact is avoided, is one of the chief excellencies of this apparatus. The cutting is done at one sweep ; where the specimen is very large, I am able to run back with the knife and make a second sweep, without materially rendering the section unequal.

I seldom harden the specimen, as completely as Gudden and his pupils do, but find that a little elasticity does no harm, and is of advantage in the subsequent manipulation of the fragile specimens. If the various fasciculi and fibrillæ should not be distinct in such semi-hardened specimens, the sections, as such, may be submitted to the influence of chromic acid or its salts.

I have found no difference between the action of bichromate of ammonia and bichromate of potash. I stain the specimens in dilute carmine (rendered perfectly neutral by exposure to the air, then repeatedly filtered), often leaving them for a week or more in this fluid. Hæmatoxylin in alum, is as applicable as carmine to the staining of a large series of sections. I must confess, that I have seen nothing in chloride of gold, osmium or silver salts which would lead me to prefer them to carmine and hæmatoxylin; on the contrary, I suspect them, like all metallic salts, of causing false appearances, by precipitation of the oxides of the respective metals.

I need not enter into the well known details of mounting the sections, which is done in Canada balsam, after preliminary de-hydrization by alcohol, and rendering the cuts transparent with ol. caryophyllæ.

The brains of smaller animals may be stained as a whole, previous to cutting in a manner similar to that employed on embryos. I usually add a neutral, and dilute solution of carmine to the hardening fluid, after the brain has been subjected to the action of the latter *alone* for a week or two. Such specimens may be cut with turpentine, and *mounted as cut.*

The object of having the sliding plate, which carries the knife in the apparatus just described, made of glass, is evident. If made of any other material, it would prevent the operator from watching the cutting process.

Before closing these preliminary remarks, and proceeding to my subject proper, I would offer a few suggestions on the extent to which anatomical data may justify physiological deductions.

Assuming the anatomy of the peripheral nerves as known, it is fair to suppose that the development of the central tubular gray matter, in other words, the nerve nuclei, will rise and sink with the development and functional importance of the corresponding peripheries. This relation is embodied in the projection-theory of Meynert, to whom we owe the greatest advance made in modern times in our field.

It finds one of its most striking confirmations in the almost total absence of the trochlearis, abducens and oculo motor nerve nuclei in the mole, and their relative atrophy which I have found in the bat.

In so far as an intellectual element plays the part of a higher controlling factor, it does not increase the development of a given area in the central tubular gray matter. Thus the hypoglossal nerve nucleus is not so much more developed in the human species, as compared with the dog, when we consider to what intricate co-ordinations the tongue is subservient in the former, as contrasted with the latter! Here the *same gray matter* is under a more multilocular cortical subjection in the human being, its relations are more complicated, its mass, however, not necessarily larger. The same statements apply to the second category of gray matter, namely, the ganglia, especially the automatic, as the thalamus and corpora quadrigemina. Again, a periphery may remain the same in a large series of animals, and yèt the central projection area, may vary exceedingly. This is the case with the cerebellum, which embryologically and morphologically is an excrescence of the auditory nuclei, just as it, physiologically speaking, is the centre for the semi-circular canals. Here, as we rise in the animal scale, the cerebellum does not develop from the simple valvule of gray matter found in the fish, to the massive folia of the arbor vitæ in the primates, because of a higher development of the semicircular canals, but because of the more manifold relations into which it is thrown. Thus the powerful strands of the restiform column, internal division of the pedunculus cerebelli, brachium pontis, brachium conjunctivum, bring it into relation successively with the general sensory periphery, the hemispheres directly, and the subthalamic region, nucleus tegmenti, and so forth. Each fasciculus of nerve fibres necessitates the increase of the gray matter; and thus we have the massive development of the latter in the mammalian cerebellum.

Ganglionic matter rises and sinks in mass with the nerve bundles which terminate in it; and when we are tracing a fasciculus to a gray area which appears adequate to receive all its fibres, and can exclude a further extension of the fasciculus by a reticular breaking-up, we are justified in concluding that the fasciculus in question terminates in that special gray area.

Some of our most valuable conclusions can be drawn from comparative anatomy; from the relative diameter of a fasciculus we can judge of its peripheral relations and often surmise its central termination. The mole, bat, cetacea generally, and the Macropidæ furnish instances of physiological atrophies or hypertrophies, which deserve a careful study. It is important, as Forel suggests, that only such animals should be compared as are zoologically related, for otherwise purely zoological differences may be erroneously classed as physiological. Of this fact Meynert had lost sight; but, barring a few erroneous surmises into which he was led by this, his principle remains unaltered, and is destined, no doubt, to materially advance our knowledge of that anatomical basis on which experimental physiology should rely for its confirmation. So firmly convinced am I of the correctness of this principle, that I do not hesitate to attribute the difference in size existing between the lobi optici of two species of American frogs, to the difference of the size of their eyes. Where the anatomical connections are once clearly established, and comparative anatomy as well as embryological development, support the conclusion, I believe that we may make physiological deductions, without resorting to doubtful experiments or impure pathological cases. That the sources of error, mentioned by Forel, should be eliminated, stands to reason; but the *principle* of the projection theory is not affected thereby, nor can all Forel's objections to Meynert's statements be sustained. If we are to insist on tracing *every* individual fibril to the cell in which it terminates, or by which it is interrupted, we bar all progress in cerebral anatomy.

(TO BE CONTINUED.)